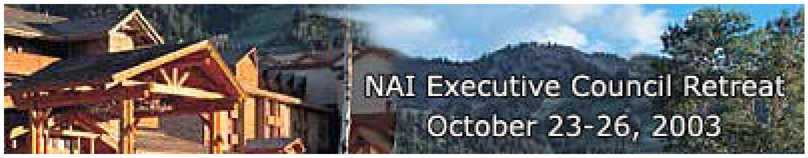


Executive Council Retreat
The Snake River Lodge and Spa
October 23-26, 2003

Abstract: Ames Research Center

NAI-Wide Initiatives in "Astro" and in "Biology"

David Des Marais and the Ames NAI Team
NASA Ames Research Center, MS 239-4, Moffett Field, CA 94035-1000

We propose collaborative research initiatives that addresses forming habitable planets and also microbial ecosystems. To support these initiatives, the new Ames NAI research effort follows a path that links investigations of the formation, evolution, and climates of habitable planets ultimately to studies of the impact of established microbial biospheres on planetary chemistry and climate. Particular attention is paid to inputs of interstellar prebiotic chemicals to nascent planets as well as the subsequent formation of detectable biosignatures by evolving microbial ecosystems.

We propose a collaborative study that will enhance our understanding of the distribution of habitable planets in the universe, particularly when performed in concert with relevant upcoming flight missions. Because extrasolar planets that host surface biospheres are the most likely to be detected by remote spectroscopic search, the Ames team focuses on terrestrial (rocky) planets where liquid water is stable at the surface, and examine a critical subset of the processes that affect planetary habitability. The research objectives for an NAI-wide effort could include the following: (1) Understand how protoplanetary disks evolve and form terrestrial planets; (2) Determine the kinds of planetary systems that are likely to harbor terrestrial planets; (3) Trace, spectroscopically and chemically, the cosmic evolution of organic molecules from the interstellar medium to protoplanetary disks, planetesimals, and finally onto habitable bodies; (4) Determine how impacts affect the climatology of terrestrial planets; and (5) Identify the particular evolutionary paths of terrestrial planets that result in habitability; and how external characteristics, such as orbital eccentricity, and internal factors, such as atmospheric circulation, affect the habitability of terrestrial planets. Ames NAI team members are actively involved in the Kepler, Stardust, and SIRTF missions and the SOFIA flight program, and thus can help to link the NAI effort to these missions. Other missions also offer opportunities to examine emerging planetary systems.

We propose collaborative studies of the major factors that govern the formation of potentially diagnostic biosignatures in microbial ecosystems. These studies should include efforts to understand the limits of microbial life, particularly in the subsurface. Ames will examine two ecosystem types for their relevance to astrobiological searches for life. Studies of rock-hosted ecosystems in ophiolite springs will examine how microorganisms might leave biosignatures by affecting the formation of aqueous alteration minerals, and how biological energy requirements define an "energetically habitable zone" for chemotrophic life. Parallel studies of cyanobacterial mats will focus on elucidating the pathways by which photosynthetic productivity is transformed into volatile biosignatures that could be distinguished in the atmospheres of distant planets. We will extend these ecosystem-level studies to a planetary scale by refining quantitative models that simulate energy relationships, biogeochemical cycling, trace gas exchange, and biodiversity in these systems. Several new NAI teams address microbial ecosystems in extreme environments, thus the potential exists for fruitful collaboration. The Ames team is involved with the Mars Exploration Rover, Mars Reconnaissance Orbiter, and Terrestrial Planet Finder, so this research can be directly linked to relevant missions.

Abstract: Carnegie Institution of Washington

Astrobiological Pathways: From the Interstellar Medium, Through Planetary Systems, to the Emergence and Detection of Life

The Carnegie Institution of Washington team entering the second five years of the NAI is broader in both membership and scope than the founding team of 1998. The CIW team consists of 27 investigators; eighteen from within CIW and nine from the American Type Culture Collection, the Naval Research Laboratory, the Smithsonian Institution, the University of California at Santa Cruz, the University of Maryland at College Park, the University of Texas at Austin, and the University of Washington.

Our theme of astrobiological pathways adopts the perspective of starting with solar system origins and moving toward the development and detection of biology. The first two integrated research tasks couple astronomical observation and theory with cosmochemical characterization of extraterrestrial materials. Boss, Butler, Chambers, Huntress, Seager, Solomon, Weinberger, and Wetherill will apply theory and observations to investigate chemical evolution in the interstellar medium, in circumstellar disks, during planetary formation, and on solar system bodies. Alexander, Cody, McCoy, Nittler, and Vicenzi will conduct analytical research on extraterrestrial samples, including meteorites and interplanetary dust particles, with an emphasis on unraveling the history of the early solar system encoded in the organic chemistry as well as seeking evidence on the form and history of solar system water.

The connection between astronomical observations, astrophysical theory, and cosmochemical measurements sets the stage for the next set of integrated research tasks focusing on prebiotic chemistry and the early sulfur cycle. Brandes, Cody, Farquhar, and Rumble will study prebiotic chemical and isotopic evolution on Earth, with a particular emphasis on the sulfur cycle and the special role of sulfur in hydrothermal prebiotic organic chemistry. A related task involves the investigations of Deamer and Hazen into possible mechanisms of prebiotic molecular selection and macromolecular organization, including the self-organization of amphiphiles and the selective adsorption of organic molecules onto mineral surfaces.

The environments tested experimentally for prebiotic organosynthesis also simulate intriguing “extreme” environments wherein life currently thrives, e.g., within the walls of deep-sea hydrothermal vent chimneys. As part of the team’s program Baross and his students are involved in an extensive study of the microbial ecosystem that exists along the extreme thermal gradients within the chimney walls. Their research connects with the ongoing prebiotic chemistry studies via the shared importance of transition-metal sulfides and components of the local sulfur cycle at deep-sea hydrothermal vents. Also elements of this task are laboratory studies of stress adaptation to extreme environments by Hemley and Scott and investigations into novel iron-metabolism by Emerson and Hauri.

Another task explores morphological, chemical, and isotopic biosignatures using a variety of novel and powerful analytical techniques by Cody, Fogel, Steele, Stroud, and Tuross. A final task addresses astrobiotechnology, specifically the procedures and instrumentation to provide in-situ life detection capabilities to future NASA missions.

The primary objective of the CIW effort under the NAI aegis is to foster novel synergistic partnerships across a variety of scientific disciplines, from geochemistry to astronomy, and from biology to planetary science. This synergy is enhanced through a strong postdoctoral fellowship program, a biweekly seminar series, and an active visiting scientist program open to all NAI members.

Abstract: Harvard University

Future Prospects for Early Earth

Ariel D. Anbar, U. Rochester (Harvard Team)

The search for life beyond Earth requires understanding the conditions under which life originates and evolves, the factors influencing the emergence of complex life, and the ability to interpret the "fingerprints" left by primitive biospheres on the geologic record or in the atmospheres of extrasolar planets. Such understanding must be informed by examination of the history of the only planet on which life is known to exist- the Earth. Hence, study of life and the environment on the early Earth is a critical aspect of astrobiology research.

The NAI can play a unique role in catalyzing such research, especially in regards to large-scale collaborative efforts. NAI focus groups provide a means of fostering such projects. Some such projects are in their early stages, and others may be worth developing via focus group or other mechanisms. The topics below are presented to familiarize new members of the Executive Council with one such activity – the Astrobiology Drilling Program, an outgrowth of the Early Earth Focus Group – and to stimulate discussion about future possibilities.

Astrobiology Drilling Progam

Despite intense interest, research into the emergence and early evolution of life, particularly in relation to environmental conditions, is limited by availability of suitable samples. Most Archean and Paleoproterozoic rocks have been altered by metamorphism, hydrothermal mineralization or modern weathering. Where classic Precambrian stratigraphy has survived such alteration, suitable exposures are often scarce due to the occurrence of flat-lying strata or sedimentary cover. This issue was identified as a priorities in the earliest deliberations of the Early Earth Focus Group.

Stratigraphic drilling using clean diamond drilling techniques, targeted in accord with scientific priorities, could provide samples of unmatched quality across the most interesting stratigraphic intervals. Following on proposals by the Focus Group, the NAI's nascent Astrobiology Drilling Program (ADP) aims to promote such sampling activities and encourage adherence to community-defined protocols for sample acquisition, archiving and distribution. The ADP is not strictly a funding program, but rather provides seed funding to be used as leverage with other institutions and a framework for oversight and coordination of such collaborative projects.

The ADP's activities begin with a coordinated investigation of critical Archean successions in Western Australia. These investigations consist of two complimentary projects: The Archean Biosphere Drilling Project (ABDP), spearheaded by researchers at Penn State University and Kagoshima University, and the Deep Time Drilling Project (DTDP), led by researchers from U. Rochester (Harvard Team), U. Washington and U. Colorado. The ABDP, which began drilling activities in 2003, aims to obtain numerous shallow drill cores from horizons of particular interest. The DTDP, which will initiate in 2004, is focused on acquisition of continuous sequences from key stratigraphic intervals.

Management of the ADP is via a steering committee. Key tasks immediately ahead for this committee include development of protocols for sample archiving and distribution. Opportunities are expected in the future for teams of investigators to propose new projects.

Earth's Earliest Biosphere On-Line

This year marked the 20th anniversary of the publication of the classic reference text *Earth's Earliest Biosphere*, a product of the Precambrian Paleobiology Research Group (PPRG) under the leadership of Bill Schopf. This text assembled the state-of-the-art knowledge of the day on the Precambrian biosphere, as revealed by geological, geophysical, geochemical and paleobiological evidence. Chapters were authored by some of the most active researchers in their disciplines, most of whom are still prominent in the astrobiology research community. *Earth's Earliest Biosphere* was extremely influential, serving as both a sophisticated primer for new researchers and as a comprehensive reference for specialists. A successor to this text, *The Proterozoic Biosphere*, was published in 1992.

In view of lessons learned in the past decade, both these texts are now dated. While still useful as primers in some of the topics covered, some topics now seen as important are not covered at all. Additionally, because of the expansion of available data they are no longer canonical reference texts. At the same time, with the rapid growth of astrobiology research enterprise the need for a resource that serves as a comprehensive primer and reference work has never been greater.

A single text can no longer serve this purpose. The challenge of assembling such a text can be seen in the expansion of page count from ~ 530 pages in 1983's *Earth's Earliest Biosphere* to ~ 1350 pages in 1992's *The Proterozoic Biosphere*. A comprehensive modern text, covering the Archean and Proterzoic, would be a multi-volume effort, would likely be obsolete before it was published, and would not capture the excitement inherent in many of the debates that characterize astrobiology research today.

A practical, forward-looking alternative would be to develop a web-based resource that serves much the same purpose, but in a 21st-century format. Such a resource might include summaries of critical topics written by specialists for non-specialists; compilation of, or links to, comprehensive databases, accompanied by overviews for the uninitiated; links to new research papers, along with summaries of their contents written by the authors for nonspecialists; topical weblogs and/or chat features designed to provide periodically-updated commentary or moderated debate on controversial topics; and resources for educators at all levels.

The value of this project would lie in the assembly of such a resource for specialists and non-specialists, and also in the relative ease with which it could be kept current (as compared to hardcopy texts). Properly managed, such a project should also be attractive to researchers as a new mechanism to efficiently communicate their findings to the astrobiology community. Ideally, such a web resource would develop into a virtual "meta journal" on early Earth topics, regularly providing state-of-the-art information to interested readers and presenting synthesis review articles updated as appropriate. This undertaking is beyond the capacity of a single part-time editor or even a small group, but seems ideally suited to be a "focal activity" for a focus group with broad community membership.

The Early Earth and Space Exploration

One of the original goals of the NAI was to have an impact on NASA space missions. Although it may seem as though early Earth research is only indirectly connected to space exploration – for example, by providing interpretative context for future data from the Terrestrial Planet Finder – more direct connections are conceivable. Lunar exploration might offers particularly interesting opportunities for early Earth research for two reasons.

First, the lunar surface provides a rich, integrated record of the bombardment history of the Earth-Moon system. Particularly before 3.8 Ga, this history is very much obscured in the terrestrial geologic record because of the inexorable consequences of tectonics and, perhaps, because of the violence of early bombardment itself. In contrast, the lunar surface offers an unparalleled record of this period – indeed, the geochronologic dating of some of this history is one of the major legacies of the Apollo program. However, fundamental questions persist about the intensity, duration and episodicity of the Earth's early bombardment history – questions relevant to understanding sources of material at the surface of the prebiotic Earth (especially volatile compounds and organic carbon) and the "impact frustration" of life's origin and early evolution.

Second, it is possible that samples of crustal rocks from the early Earth can be found at the lunar surface, just as lunar and Martian samples are found on Earth today. Finding such rocks is akin to finding a needle in a haystack. However, if found, these materials could provide unparalleled insights into environmental conditions around the time of life's origin on Earth, and perhaps even into the nature of early life itself.

With many voices calling for NASA to define new goals, particularly for human spaceflight, there may be a unique opportunity to advance astrobiology research goals in the context of robotic and crewed exploration of the Moon. Examination of this concept may be a useful new activity for the Early Earth Focus Group, or for a new astrobiology focus group centered on lunar science or human spaceflight.

Abstract: Indiana University (Bloomington)

Exploring Deep-Subsurface Fractures for Life

During the past five years, our understanding of biodiversity, geochemistry and isotope systematics in the deep subsurface of Earth has advanced rapidly. Microbes have been recovered from several types of fractured rock but culturing these organisms remains problematic and pathways of electron transfer are enigmatic for both the chemoautotrophic and heterotrophic microbes in deep-subsurface ecosystems. Limiting factors for microbial life trapped within rock for millions of years are not known with certainly but are assumed to be nutrients and/or chemical gradients necessary for fundamental cellular functions such as ATP synthesis, DNA repair, protein production, and maintenance of membrane charge. Given the rudimentary state of knowledge, what chance do we have to detect a cell that hasn't divided in a thousand years and to recognize it as a living organism?

Life-forms in the subsurface of other planets are presumed to concentrate energy from geological sources similar to those in Earth's subsurface, although the cellular machinery of non-terran organisms could be radically different. Do elements of commonality exist in the utilization of energy resources by terran organisms that can be extended as a basis for searching for life in the subsurface of Mars or the outer planets? Laboratory studies of microbial and abiogenic processes are essential for understanding the reservoirs and fluxes of biosustaining energy and nutrient cycles in the deep subsurface. These experiments should include the expression of genes and detection of metabolites in microbial macrocosms replicating a spectrum of subsurface conditions. How can we ground-truth laboratory studies when access to the deep subsurface is severely limited? Commercial mines generally are reluctant to collaborate with scientists and there are serious risks associated with sending researchers into deep mines at times when fluids are intersected. Only a few sites below 1.5 km of depth have been studied with aseptic techniques and, as a consequence, we are limited in our ability to build accurate models of subsurface life on Earth and to refine our exploration strategies for Mars.

Astrobiology desperately needs a probe small enough to swim or crawl through cm-scale boreholes, probe mm-scale fractures and return with measurements and samples of rocks, fluids, and microbes. Small fractures probes could be release from a drill stem at levels where fluids and gases are intersected during conventional drilling. Probe measurements would minimize contamination of the fracture network by direct drill sampling and would enable rapid expansion of our knowledge about deep-subsurface microbial ecosystems. With modifications such as automation, a fracture probe could be flyable to other planets. This type of probe would have considerable commercial value in the petroleum and environmental industries. Probe development would require a high degree of collaboration between technological firms, NASA engineers, and geologists. Field testing on Earth would require diverse geological and environmental sites where such a minisub could be deployed and where probe measurements could be verified by convention extraction methods.

Abstract: Marine Biological Laboratory

A Genomic-Based Approach for Linking Earth's History with the Evolution of Metabolic Function

Microbes (Archaea, Bacteria and Protista) were the only living things for most of the history of the Earth. Over 3.5 billion years ago they began the process of transforming this planet through microbial mediated biogeochemical processes, making it habitable for multicellular organisms that dominate our visual landscape. If life exists elsewhere in the universe in a recognizable form, it is likely to be in the form of complex microbial communities that have changed the atmosphere and surface of its habitat. Any attempt to understand the impact of microbial life on earth (and elsewhere) requires a detailed understanding of the evolutionary history of all microbial lineages, including the origins and elaboration of key biological innovations. For the past twenty years, analyses of ribosomal RNAs have overhauled and dominated our perspective of microbial evolution. Molecular evolutionists have generated a forest of phylogenetic trees for a vast array of eukaryotic, archeal and bacterial microorganisms, some of which survive in environmental settings once considered to be incapable of supporting life. More recently the wide spread application of molecular analyses to different gene families has produced phylogenies that all too often conflict with those inferred from analyses of rRNA molecules. This apparent discrepancy among phylogenies based on different genes has fueled debates surrounding the frequency and breadth of lateral gene transfer among microbes, and leaves many to wonder if a reliable consensus tree of life will emerge from more complex data sets.

With today's rapidly growing data bases of complete or nearly so genome sequences, it should be possible to systematically construct phylogenetic relationships for divergent organisms and to evaluate whether or not genes can faithfully describe our evolutionary history. Comparative genome approaches are at a very early stage of development and they most commonly evaluate similarities between genomes based upon gene content rather than detailed phylogenetic analyses of individual genes or concatenated gene sets. The absence of aligned gene sequence data-bases for all recognizable homologues in a data set of several hundred genomes reflects the absence of automated tools for curating divergent sequence data sets and computational time required to infer phylogenetic trees. To address this problem, the NAI could mount a task force of students, post doctoral fellows and principal investigators who would partition out the curation of all phylogenetically conserved coding regions in 200-300 microbial genomes that represent known diversity within the tree of life. This would entail the construction of data bases for 2000-3000 gene families and the identification of regions that are unambiguously aligned. The NAI could implement this task by building a virtual data base. The use of consensus phylogenetic inferences and supertree-building technology would be applied to this data set to address the following major questions:

- Can phylogenomic approaches report a robust tree of life?
- To what extent is our evolutionary history a dichotomous tree versus a net-like structure that reflects frequent lateral gene transfer?
- Can we identify within a consensus tree of life the origins and timing of novel metabolic activities that had a major impact in planetary transformations, as preserved in the geological record?

Efforts in this emerging area of genome science are still limited in scope, and all suffer from lack of human resources to curate the alignments and to build and evaluate trees. However, first attempts to build genomic trees based on shared gene content of widely divergent organisms reveal the promise of these approaches (House et al. 2003). Human resources and scientific expertise are required for identifying functional domains within proteins, weighting alignment parameters in a manner appropriate for specific genes, and identifying paralogs that reflect ancient gene duplications and confound phylogenetic reconstruction. These tasks cannot be accomplished with computational tools alone, but could be tackled in a distributed manner that would engage part-time efforts of several hundred members of the institute. A comprehensive phylogenomic reconstruction of life, together with the geological record (Knoll 2003), aims at nothing less but a complete understanding of the mutually interdependent biological and chemical evolution of this planet. It would provide a platform for interdisciplinary training and most importantly lead to new and exciting interpretations that would be uniquely astrobiological in content.

House, C. H., B. Runnegar, S. T. Fitz-Gibbon. 2003. Geobiological analysis using whole genome-based tree building applied to the Bacteria, Archaea and Eukarya. Geobiology 1:15-26.

Knoll, A. 2003. The geological consequences of evolution. Geobiology 1:3-14.

Abstract: Michigan State University

Genomic and Evolutionary Studies on Microbial Life at Low Temperature

Our overall Research Interest

One of the three major goals of NAI is to understand how life begins and evolves. Our research team is interested in understanding how microorganisms adapt to environmental changes that occur during a planet's history. In particular, we are interested in the adaptations and evolutionary processes that enable microorganisms to cross niche barriers. The specific niche barrier that we are focusing on is low temperature. This environmental condition is of particular relevance to our planet as more than 80% of the biosphere is permanently cold (normally below 5ºC). In addition, it is generally believed that life initially arose on a hot planet Earth which then cooled. How did microorganisms adapt to and invade new low temperature environments? These questions are relevant beyond Earth as low temperature is a common theme throughout the Universe and our solar system, including Mars and Europa, where life may have existed (still exists?) in the past.

Approaches

We are conducting "test tube evolution" experiments to see how a mesophile (Escherichia coli), with a given gene complement, can adapt to cold/freezing temperatures. In addition, we are studying permafrost bacteria to determine whether low temperature fitness primarily reflects quantitative adaptations (i.e., selection for cold tolerant gene alleles) or whether qualitative adaptations (i.e., novel genes that enable organisms to cope with low/freezing temperature) have a significant role. To address these issues, we are integrating the use of a variety of "–omic" technologies including genome sequencing, transcriptome analysis, proteome analysis and metabolic profiling. We regard these approaches as being areas of expertise that we bring to the NAI.

Proposal for Consideration

Understanding how microorganisms adapt to low temperature (with emphasis on temperatures below freezing) is important to astrobiology as the topic relates to how life evolves, the extremes at which life can exist, and the interplay between changes in a plant's physical properties and resident biota. To advance this line of investigation, we propose a genome sequencing initiative to obtain the genetic blueprints of for a number of microbial genomes. Of the more than 150 bacterial genomes that have been sequenced, only 4 are of psychrotrophs. Our view is that knowing more about the genomes of bacteria that have been subject to natural selection at cold temperatures and are adapted to low temperature environments is key to progress on this topic. The NAI research teams have a broad range of expertise and capabilities to isolate microorganisms from a wide range of cold environments (deep sea; permafrost; etc.), to characterize them phylogenetically, and to choose key species for sequencing efforts. We envision a bacterial sequencing initiative centered on life at low temperature being a component of the "research portfolio" of NAI.

Abstract: NASA Goddard Space Flight Center

The Organic Origins Observatory

Drake Deming, Charles Bowers, Michael DiSanti, Jason Dworkin, John Mather, Michael Mumma, Joseph Nuth, Bruce Woodgate, and the Goddard Team

A radiatively-cooled 2-m telescope in space equipped with a high-spectral-dispersion infrared echelle-grating spectrometer (R=75,000) and a slit-viewing camera would enable unique Astrobiology-related science. It would permit abundance measurements for several dozen distinct chemical species, along with their ortho-para ratios, isotopic and isomeric abundance ratios, etc. The principal object classes would be disks around young and evolved stars, exoplanets, and comets in our solar system. The objective is to quantify the chemical classes of icy planetesimals, and to assess their role in delivering water and pre-biotic organics to planets.

Some Key Scientific Studies

Origin and evolution of pre-biotic organics
comets (chemical taxonomic classes; internal heterogeneity; D/H ratios, etc.)
disks around stars (in emission and absorption, hence both ices and gases)
proplyds (photoevaporating protoplanetary disks)

Exoplanets properties (in emission, and in absorption when transiting)
atmospheric signatures (H3+, CH4, H2O, H2, etc.)
atmospheric loss cones (tails), circumstellar tori, rapid exoplanet detection

Some Key Technical Aspects

Improves detection limits for emission line sources:
Maximize spectral contrast of emission lines vs. thermal continuum
Sensitivity improves as R0.5, speed as R, in continuum-limited case.

Improves spectral separation, enabling greatly enhanced chemical specificity, OPRs, isotopic and isomeric ratios, etc.
Gases: H2O, CO2, …
CO, H2CO, CH3OH, …CH4, C2H2, C2H4, C2H6, C3H8, …
NH3, HCN, HNC, HC3N, N2H4, etc. HDO, CH5D, C2HD, C2H5D, …

Measure ices in presence of gaseous emissions: H2O, CO2, CH4, etc.
Polar vs. apolar ices from shape of ice absorption feature

Velocity profiles of spectral lines: 2 km/s sampling, 4 km/sec resolution.
Circumstellar disks, exoplanets and associated tori
Measure vorbsin(i) (from shape of spectral lines)
2 km/sec orbital velocity occurs at 225 AU (solar-mass stars)
Estimate astrocentric distance (Rh) (Trot line-by-line intensities); get sin(i).
Can provide major advantage over stellar radial velocities (mass ratio, specificity)

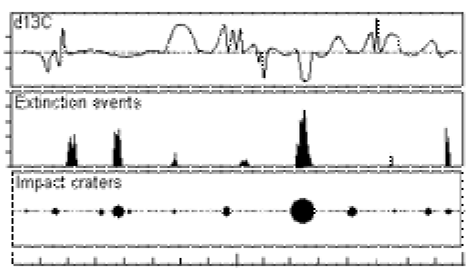
Abstract: Pennsylvania State University

Planetary History Database

Goal: the discovery of new patterns and mechanisms in the evolution of life and development of the biosphere through the assembly of multidisciplinary, time-based data.
Potential Focus Group involvement: Mission to Early Earth, Evogenomics, Impacts, Mars, and Europa.
EPO content: this will be a public database for use at all educational levels.
Coordinator: Blair Hedges, Penn State.

Overview: A vital part of astrobiology research is to relate and compare information from diverse fields such as paleontology, geology, geochemistry, and molecular biology in the same timescale. Currently no database provides this function. The *Planetary History Database* will pull together biological and abiological information in a single timeline to make those data more accessible and facilitate the discovery of patterns. In turn this will help identify mechanisms that shaped the development of life on Earth and possibly elsewhere. The database will also be an educational tool accessible over the Internet. A committee of representatives from the focus groups and lead teams will oversee the database.

Background: Major advances in science have come from comparison of diverse data in the same timescale. Examples include continental drift (fossils, position of continents, magnetic anomalies), the Cretaceous-Tertiary impact (rare elements, dinosaur extinctions), and the Snowball Earth hypothesis (C-isotope excursions, glacial deposits). Such comparisons are integral to astrobiology as noted in the NASA Astrobiology Roadmap "Investigate the historical relationship between Earth and its biota by integrating evidence from both the geologic and biomolecular records of ancient life and its environments" (Goal 4). However, a geochemist may have difficulty finding a phylogenetic tree of protists and a molecular evolutionist might have difficulty locating sulfur isotope data. This is a proposal to construct an Internet database that maintains the data most frequently used by astrobiologists and displays them together in the same timescale.



Data will be presented as horizontal panels scaled to time, with the user having the option to choose which panels to display at one time (e.g., C-isotope, S-isotope, and impact events; or C-isotope, phylogeny, and extinction events, etc.) and the time frame. The user may click on a data point (e.g., S-isotope value or impact event) and obtain detailed information in a pop-up window or mouse-over. This database is not intended to compete with field-specific databases, such as the *Paleobiology Database*, because not all information useful to specialists in a field will be needed in the *Planetary History Database*. For example, first occurrences of groups in the fossil record and the timing and nature of extinction events are relevant data, but not necessarily <u>all</u> fossil occurrences. Besides data panels, model panels (e.g., atmosphere evolution) will be available. Most panels will pertain to the history of Earth's biosphere, but panels for Mars and Europa will be included, facilitating comparison of planets and satellites in one timeline.

Scope and resources needed: It is appropriate for the NAI to oversee such a project, but determining the scope of the data collection and resources required will require discussions with NAI members.

Abstract: SETI Institute

M-Star Planet Habitability and Consequences for Next-Generation SETI Searches

Rocco Mancinelli, Christopher Chyba, Peter Backus, and Jill Tarter, SETI Institute

The SETI Institute has partnered with the UC Berkeley Radio Astronomy Laboratory to design and build the Allen Telescope Array (ATA). When completed, this instrument (an array of over 350 6 m dishes) will be dedicated to a targeted SETI search of nearby stars while simultaneously being used to do traditional astronomical surveys and research. The key to this dual-use strategy is a large list of target stars that represent credible abodes for technological civilizations i.e. that are “habitable” from the point of view of complex life. The ATA will have a very large instantaneous primary field of view (PFOV) (because the array is constructed with small 6 m telescopes) and, if the target list is big enough, then on average several SETI target stars will be contained in the PFOV. The ATA will have a unique signal processing architecture permitting multiple SETI stellar targets to be observed while the PFOV is also being imaged for astronomical studies. The work being proposed here is an expanded investigation included in our NAI proposal that concentrates on the scientific question of what constitutes a ‘Habstar’ (those stars most likely to be suitable hosts for habitable planets), criteria for recognizing them, and in particular, the long-standing debate over whether M-stars should be considered among them. These results will be directly relevant to eventually expanding existing target lists to 10^6 stars that are good candidates for SETI observations from the ATA.

Turnbull and Tarter have recently published a catalog of 17,129 ‘Habstars’ based on the point source catalog from the Hipparcos mission, and expanded that to ~250,000 stars using the Tycho-2 catalog (substituting reduced proper motions for unmeasured distances). We will continue to expand the list of target stars in stages, by using the existing catalogs of point sources, and those that are now becoming available from ground-based observations. Important for the construction of the million-target star catalog is the selection (or not) of M stars since they comprise about 70% of the nearby stellar population. This question brings together the work of many members of our NAI team with that of other colleagues in the NASA Astrobiology Institute, to inform the decision of which, if any, M stars should be included as ‘Habstars’.

Since the beginning of observational SETI programs, M stars have been excluded from most searches for three reasons: The habitable zone is so close to an M star that any planets would be locked into synchronous rotation (with potentially dire consequences for any atmosphere); the flaring activity of these stars during the first few Gyr of their lives would provide an untenable UV environment for the development of complex life on the planetary surface; and star spot activity would lead to unsuitable fluctuations in the total stellar luminosity. All three of these assertions may be questioned.

We propose a series of two focused workshops separated by 12 to 18 months to permit tangible results to appear from work done during the interval. The workshops will be held at the SETI Institute during 2004 and 2006, and be co-chaired by Drs. Tarter and Mancinelli,. Many SETI Institute NAI team Co-Is will participate, but we wish also to invite participants from other nodes in the Astrobiology Institute. The workshops will include two dozen scientists with specialties in particular microorganisms or atmospheric and planetary modeling. Participants will be presented with the most recent “climate” calculations for a planet within the continuously habitable zone of an M star as well as the temporally varying biologically relevant UV surface fluxes calculated by other workers. The participants will be challenged to validate or refute these environmental conditions and to show whether the organism or radiation-damage repair mechanism in which they specialize would be viable under these conditions. Results from the first workshop will feed into the second workshop, at which participants will attempt to predict mutation rates and sensitivity to extinction. The goal of this latter exercise is to derive possible constraints on the frequency, duration, and magnitude of M star flaring events that can be used to answer the important question of whether M-star planets which if they exist could be the most abundant planets in the galaxy, since 70% of stars are M-type provide credible sites for life or even intelligent life. These results will in turn have direct implications for determining observational strategy for the next generation SETI search.

Abstract: University of Arizona

Planning the Future of Astrobiology (A View from Tucson, Arizona)

It is likely that in the next 25 years we will either have created laboratory life on Earth, or found evidence of present or past life elsewhere, or both. At that time we will urgently need to continue the work, and unless we have fully planned now for the researchers and their evolving need for resources, neither will be available.

We can observe today the confusion that results from disciplines being separated. Each discipline tends to overextend itself as a prime source of processes, because the practitioners do not have the needed background in other areas. Even when workers try to quote from those outside their field, there is a tendency for distortions or errors to enter.

These are reasons why we are trying to train astrobiologists. We hope they will be scientists that can put processes in the context of the evidence from a wide range of sources. But where will they find their future work homes? Where will they find their needed resources? Will single discipline departments want to take them in? Who will judge the quality of their work? How will they get emotional and scientific support for their work ?

The difficulties of the planning process begins with lack of resources. As one looks at the cost of future missions to look for life elsewhere, one cannot fail to be struck at the ratio in the cost that goes into the engineering as compared with the effort going into the science issues raised by the missions. (Science related engineering issues tend to be better served.) Can this possibly be optimum?

Although NASAs (time-variable) goals indicate that searching for life and understanding its origin are a high priority goal, the technological aspects of missions tend to get developed without a corresponding development of the science. We all understand that the technical difficulties associated with missions make it very difficult to optimize the science. Yet there is rather limited feedback between design engineers and scientists. Very rarely do the science issues create a precise set of technical requirements, and the interface provided by the formulation of the requirements often act as a barrier to revision in the light of first attempts to make a total design. Astrobiologists who have a broad cross0disciplinary understanding of the science end of the trade space could make a better interface to optimize missions. This is another reason for needing them. So, how many astrobiologists are needed by 2025?

Other questions that follow involve administrative issues. Who will be expected to pay their salaries then - and between now and then? And if the organizations are not NASA centers, why will they want to bring astrobiologists on to their faculties? Where will the new researchers find their research funds, summer research opportunities, research tools, research space, research leaders? Will funds be available to support even the graduate students now in the pipeline after their thesis work is completed? Is it moral to graduate students into astrobiology at the present time without having a plan? Or are we just paying lip service to astrobiology, and really focussing our efforts on turning out another generation of specialists?

And how should we train astrobiology graduate students, after we have sorted out how many we will need? It has been argued in the creation of NAI that we cannot bring to one place enough astrobiology, and so we need a spread out NAI. Is that so true that we cannot train graduate students without help from one another? Or is it that we need the links for top level research, but the educational component could be adequately handled within the current institutions?

How do we get the right courses? If we need astrobiologists, we have to make their path easy and short. Yet if we live with current discipline courses, the graduate education will be weighed down by courses in biophysics, biochemistry, planetary astronomy geology, evolutionary biology etc. as well as a comprehensive set in an area of specialization. And why should a student take all those when it is currently so much easier to work within one discipline?

Currently at the U. of Arizona we are planning to try to run a graduate "Winter School", where we take 10 students from within UA, and invite (and fund) 10 students from outside, to spend one entire semester January through April in Tucson,. We will also invite a few faculty members from other NAI centers. We can provide hands on training experience utilizing astronomical facilities that students are unlikely to find available elsewhere. But will other universities departments be willing for their students to be gone for one semester? Will they accept credits from us? Would we be willing to let our students spend a semester at an equivalent school that has a different emphasis - say marine biology or vulcanology ? It is new territory. Should we be setting up a consortium of universities to share astrobiology students? And should that sharing extend to institutions that do not grant degrees? Should we have a rotating training program that runs a student through two or three different institutions before they start their thesis work? And if so how will they gain the specialized knowledge they will need during their thesis work?

We need agreement. And then we need a plan. And then we need implementation.

Abstract: University of California, Berkeley

BIOMARS: The new UC Berkeley-led NAI

Jill Banfield, PI. Department of Earth and Planetary Science, UC Berkeley.

Mars is an exciting, and comparatively accessible target for astrobiological studies aimed at detection of current or past extraterrestrial life. Our NAI team will analyze the evolution of the Martian hydrosphere and surface topography to understand the history of water distribution and investigate atmospheric processes that may have contributed to a UV shield. Our objective is to identify the types of sites on Mars that experienced long-term fluid flow as these may be, or have been, conducive to life. We will develop models for Mars planetary evolution to constrain the timing and scale of hydrosphere development and subsurface water circulation. We will couple these hydrosphere models to geomorphological models based on terrestrial field site analyses and experimental studies to allow detailed interpretation of Mars surface features. This will permit analysis of the history, form, and timing of fluid flow events that shaped the planetary surface and determination of the factors that control them. In parallel, we will explore atmospheric processes that could have contributed to a UV shield and conduct spectroscopic studies to constrain Mars surface mineralogy.

Life on Mars may have developed in redox gradients between reduced basaltic rocks or minerals and oxidized fluids and/or gases. Element cycling in these systems could underpin (or could have underpinned) a substantial biosphere. We will characterize biomes that develop in Earth environments chosen to resemble potential Martian habitats. We will focus on terrestrial chemoautotrophically-based ecosystems at sites of groundwater discharge in basaltic rocks and habitats in volcanically-hosted metal sulfide-rich deposits as these appear to offer the best combination of energy sources, sustained water flow, and protection from UV radiation. We will characterize these habitats in terms of their population structure, aqueous geochemistry, mineralogy, and isotopic signatures. Parallel laboratory-based studies will explore the ranges of temperature, concentration, and pH consistent with life in these habitats and biochemical analyses will explore the factors that set these limits. We will analyze the structure, elemental and isotopic composition, microstructure, morphology, and distribution of minerals generated by, or impacted by, life in these rock hosted systems so as to develop and test potential new biosignatures. Parallel inorganic experiments will be conducted in order to resolve non-biological features and to examine changes in mineralogical biosignatures with time.

We will carry out robot-based sampling and in situ analyses of terrestrial sites so as to develop methods for dealing with the challenges of remote geomicrobiological investigations. Our work will provide constraints for selection of optimal sites for future Mars exploration and methods for sample analysis.

Abstract: University of California, Los Angeles

Factories of Organosynthesis as Collateral Consequences of Planet Formation: Astrochemistry in Protoplanetary Disks and Aqueous Geochemistry in Planetesimal Environments

Edward D. Young, UCLA

This short presentation will present the argument that understanding the photochemistry within protoplanetary disks and the aqueous geochemistry that occurs in asteroids (planetesimals) may be key to understanding the likelihood that life is common in other planet-bearing stellar systems. The talk will present some of the work done at UCLA while at the same time calling upon a community wide interdisciplinary approach to elucidating the importance of these potential "factories of organo- synthesis."

Abstract: University of Colorado, Boulder

Collaborative Directions for the NASA Astrobiology Institute

Bruce M. Jakosky and the CU Center for Astrobiology, University of Colorado, Boulder, CO 80309-0392 (bruce.jakosky@lasp.colorado.edu).

We would like to suggest the following strategic directions for the NAI. Each one would address fundamental questions in astrobiology that cross discipline boundaries, and each would involve interdisciplinary collaborations between individuals both from multiple lead teams and from outside the Institute. These could take the form of research-oriented Focus Groups or of collaborative investigations that may involve proposals to the Strategic Development Fund.

Sources of Energy for Chemosynthetic Life

For planetary bodies with available water and elemental nutrients, suitable sources of metabolic energy may be the ultimate factor limiting the presence, abundance, and distribution of life. In recent years it has become increasingly appreciated that a number of biological communities exist with little or no input from photosynthesis, relying instead on chemical forms of energy (i.e., chemosynthesis). These communities may provide the best analogs for communities that might exist or have existed on Mars, Europa, and Titan. As such, knowledge of the kinds of energy sources that support these communities and their spatial and temporal distribution should prove valuable for understanding planetary habitability and for guiding the search for life elsewhere. We can make progress by understanding chemosynthetic systems on Earth, how chemical sources of energy arise on the Earth, how biological communities take advantage of them, as well as the physical and chemical environments on other planets. Progress can be made by collaborative efforts of microbiologists, geochemists, geologists, and planetary scientists.

Study of the Deep Biosphere

Knowledge of biodiversity on Earth and of the functional basis of marine, soil, sediment and deep rock ecosystems is of fundamental importance to astrobiology. The least understood biome is the "Deep Biosphere," regions in Earth's crust that are thought to harbor significant biomass. Its extent and origin have implications for global biogeochemical cycles and the interactions between the biosphere and the lithosphere, the potential for discovery and isolation of new organisms adapted to extreme environments, and the potential for life to exist in similar environments on other planets. We know that a Deep Biosphere exists, but the extent and diversity of the microbial life there are poorly known. Deep subsurface life may depend primarily on metabolisms based on CO_2, such as methanogenesis and acetogenesis. Additionally, elevated burial temperatures can activate inorganic compounds such as iron minerals providing new electron acceptors. Ferrous iron minerals produce H_2, via Fe^{2+} oxidation coupled to reduction of H_2O. Hydrogen is an energy source for autotrophic bacteria that could potentially form the basis for a Deep Biosphere. How diverse is the life in this hidden biosphere? Are eukaryotes (e.g., fungi) present? What new organisms might be down there? This effort is closely related to the previous one on sources of energy, and would involve microbiologists, geochemists, geologists, and planetary scientists.

Origin and Early Evolution of Cellular Life

The origin and early evolution of cellular life is a question of critical importance. Compartmentalization was a necessary step in the evolution of life, as all living organisms are surrounded by a membrane or cell wall. It was probably an early event that fostered the development of life by concentrating and protecting small and large molecules. Understanding the emergence of compartmentalized proto-cells requires understanding how lipid membranes self-assemble from materials available in the environment and how they can divide to give rise to daughter compartments, mechanisms for transporting nutrients into and waste products out of compartments, mechanisms for utilizing membranes to couple energy from light or chemical gradients to metabolic processes within the compartment, the nature of the macromolecules inside the compartment, and the mechanisms by which proto-cells with the most effective collections of macromolecules would come to dominate a population. There is strong interest among both theorists and experimentalists in developing proto-cells as models for the emergence of life on earth.

Identification of Biosignatures

Without a clear understanding of what is required for an observation or measurement to qualify as a reliable biosignature, it is difficult to reach scientifically convincing conclusions about the presence or absence of life in a sample or environment. This problem has become very apparent in the recent examples of interpretation of features in ALH84001 and the re-examination of the fossil and isotopic evidence for life in the early Archaean on Earth. Much additional thought needs to go into this issue before the validity of particular biosignatures is widely accepted, either for terrestrial analysis or for Mars or elsewhere. Recent efforts led by groups within the NAI have begun to make progress in these areas, but we are still a long way from having a convincing set of unambiguous biosignatures available. A focussed effort in this area has the potential to make substantial progress, especially if it involves collaboration rather than the antagonistic debates that have become all too common. A combined effort might involve microbiologists, paleontologists, geologists, geochemists, and planetary scientists, for example.

Development of Astrobiotechnology

As the business of NASA is, in many ways, experimental space science, the pursuit of astrobiology requires space flight experiments that can elucidate the issues involved. As this is a new direction for NASA, much of the astrobiology community does not have the long history of involvement with the flight program that would allow it to take full advantage of the opportunities. An effort focussed on developing the connections between the science and the technology that can address the science would help the community to come up to speed quickly. A focussed effort in this area would be of high value, bringing together the academic interests in astrobiology, the technology development that is taking place both in academic and in industrial settings, and the programmatic drivers that steer us in particular directions. This combined and integrated approach is beginning to happen, but without the participation of the NAI. The NAI, however, has within it the necessary expertise to make substantial contributions in this area and to be a leader in the program. We would like to put forward again the suggestion of a Focus Group that would work to enhance communications and provide national guidance and oversight in helping to develop these areas.

Abstract: University of Hawaii, Manoa

Remote Sensing Remote Observing Facilities for Interdisciplinary Research and Outreach

Water is the medium in which the chemistry of all life on Earth takes place. Water is the habitat in which life first emerged and in which much of it still thrives. Water has modified Earth's geology and climate to a degree that has allowed life to persist to the present epoch. In Hawaii, we are building a research framework that links the biological, chemical, geological and astronomical sciences to further understand the origin, history, distribution and the role of water as it related to life in the universe. Our framework will connect research on major aspects of planetary water, including observations of water in the ISM, D/H ratios and chemistry of small icy bodies in the outer solar system, cosmochemical studies of aqueous processes in meteorites, studies of diversity of rocks and sediments on the terrestrial planets and the water-rock interaction on Earth; and biological exploration of subglacial ice-covered habitats and fumarolic habitats, as well as the biological and chemical exploration of extreme environments. One of the difficulties is in connecting these diverse fields to make a truly interdisciplinary effort has been in elucidating the common research links between biology and astronomy.

Funding opportunities exist for infrastructure development and improvement - especially for innovative cross-disciplinary endeavors which would greatly enhance this effort, but the competition is intense. The intellectual resources of the NAI could be utilized to develop a unique research and EPO partnership for the institute. We would like to create a network of facilities which would allow interdisciplinary access to a network of remote observatories for research, education (including distance learning), and outreach. These would be remote control data centers accessible to a classroom environment which would allow students and research collaborators to participate in real-time scientific investigation. Some of the research and educational facilities which we envision would be controlled by such centers would include the following:

Remote astronomical observing is becoming more utilized in the astronomical community, yet the infrastructure has not quite caught up. Often, remote observing is accomplished with use of a computer workstation and video or telephone link. Observing efficiency would be greatly enhanced with a remote control room facility similar to what is present at the telescope. However, such a facility could be set up within a classroom setting which would allow student participation in the research experience. In addition, the University of Hawaii has recently entered into a partnership with the Faulkes Telescope Corporation in the UK, and the commissioning of a new 2-m research grade telescope for Education and outreach, the Faulkes Telescope, is nearly complete on the island of Haleakala, Maui. This remotely operable facility should be ready for use in late 2003, but the outreach program is just beginning, and access to a remote control center would facilitate real-time data acquisition.

The National Academy of Sciences Ocean Studies Board (NRC, 2000) recently recognized numerous significant scientific challenges that can only be addressed using autonomous Ocean Observatories. This is particularly true for seafloor and subseafloor environments where cabled or full ocean depth moorings will provide two-way communication between observatory instruments and shore-based laboratories. For example, at least one of the major mid-ocean ridge observatory sites of the National Science Foundations RIDGE 2000 program will likely be serviced by an extensive fiber optic cable system. Other sites will likely be equipped with satellite-transmitting moorings. Several UH researchers from the Departments of Oceanography and Geology and Geophysics are involved with Ridge 2000-related research. With the funding of a cable from Hawaii, a remote control center where data and from the undersea observatory could be visualized would signficantly enhance both research and astronomy efforts.

In order to understand the role of water in astrobiology, and to draw connections both on and off planet Earth, we need to study Earth as a system. Kaneohe Bay on the island of Oahu represents one of the most significant bodies of coastal water in the Hawaiian island chain. Kaneohe Bay is the largest sheltered water body in the Hawaiian islands. Erosion of the Koolau volcano produced an extensive alluvial coastal plain with fringing and barrier coral reefs, and island subsidence and Holocene sea level rise inundated the estuarine portions of the watershed, creating a shallow (8 m), well-protected bay. Hawaii is the only U.S. state located in a region in which tropical weathering processes are very important. Most studies of land-coastal margin nutrient dynamics come from temperate and higher latitude regions, although most of the suspended load containing reactive phosphorus and silicon of the world's rivers is from tropical/sub-tropical regions. Upgrades of the existing network and installation of two additional monitoring stations to be completed over a 5 year period to create a biogeochemical connection into the bay. One of the significant features of the new monitoring network would be the addition. If near real-time transmission of data from the monitoring locations to a central point for data analysis and archival research.

We will discuss the potential for remote observing centers to be used for both research and outreach for the NAI.

Abstract: University of Rhode Island

Collaborative Exploration of Subsurface Life

We would like to see the study of subsurface life further strengthened through cross-team collaboration, international partnership, and community involvement. Study of subsurface life is of significant astrobiological interest for multiple reasons. It's interesting in its own right. It provides model ecosystems for studying the limits to life, the nature of non-photosynthetic life, and life on early Earth. It provides a technical testing ground for the search for life on other planetary bodies in this solar system (e.g., Mars, Europa), which must focus closely on subsurface environments.

The study of subsurface life is the principal research focus of our NAI team. We led the first ocean drilling expedition dedicated to the study of subsurface life. We're documenting subseafloor habitability. We're detecting new phylogenetic lineages of deep-subsurface prokaryotes. We're determining categories and rates of subseafloor prokaryotic activities. We're developing techniques for documenting low levels of activity and low-biomass community composition. We're working with members of the Penn State and Carnegie NAI teams, respectively, to further understanding of subseafloor life and sublacustrine life. We're looking forward to working with members of other NAI teams to explore other aspects of subsurface life.

The study of subsurface life is an open frontier. If it is to be quickly and effectively advanced, other researchers with additional interests and expertise need to be brought into it. New approaches need to be developed for continuous monitoring of subsurface environments and life. New techniques need to be devised for gaining access (without contamination) to subsurface environments on Earth and other planetary bodies. New partnerships need to be sought for access to a greater range of Earth's subsurface environments.

Many of these techniques, approaches, and partnerships can be advanced by closer cooperation between existing members of the NAI and its international partners. For example, in situ monitoring of subsurface communities could be initiated with modification of environmental gene chip technologies that are being developed by the Centro de Astrobiología (España) and by NAI members. Study of continental subsurface life could be promoted by integration of deep-biosphere objectives into Astrobiology Drilling Program (ADP) projects that are principally focused on recovering records of Earth's early history.

We encourage the NAI to take concrete steps to advance cooperative study of subsurface life. These steps could include: (1) development and NAI-wide distribution of a list of technologies that are used or under development by NAI members and NAI partners for in situ monitoring of extreme life and environments, (2) explicit consideration of subsurface life research in development of ADP proposals, (3) sponsorship or co-sponsorship of a mobile laboratory for contaminant-free sampling and analysis of subsurface life and conditions in diverse environments (such as continental drilling projects and expeditions to over-pressured marine and continental boreholes), and (4) sponsorship or co-sponsorship of a sample repository for studies of subsurface life. Any facilities sponsored by the NAI should be available for subsurface studies by the entire NAI community. All of these steps could be initiated by NAI within a year. Development of a complete mobile laboratory and/or a fully developed repository could be spread over multiple years.

Abstract: University of Washington

Studying the evolution of biospheres

Presented by Peter Ward

Research in the astrobiology program of the University of Washington Team is an integrated, multidisciplinary effort that is concentrated on four broad questions concerned with planetary habitability and evolution of biological complexity:

How often do planets with truly Earth-like properties form?

How important is plate tectonics in the formation and maintenance of metazoan life?

How important are mass extinctions for the evolution and extinction of complex life? Is mass extinctions fertilizer or poison (or both) in the garden of complex organisms?

What are the evolutionary pathways by which complex organisms originate from microbes? Here we will examine one aspect of this research.

Earth System Science is a growing and productive field emerging from debates as to whether or not the Gaia Hypothesis is viable. We now know that the cycles of carbon, nitrogen, sulfur and phosphorus have changed over time on Earth, and continue to do so as the planet ages and its biota evolves. As we learn more about these systems we can increasingly understand now just how our earth interacts with life, but more general rules about habitable planets beyond earth.

One way of studying these systems is by observing natural "experiments" or short-term perturbations in the geological past. The most profound of these are the past mass extinctions, times when significant proportions of the Earth's biota were rapidly killed off. As part of our NAI program at the University of Washington we have constructed a laboratory for studying light stable isotopes. Using these new tools, we have been examining three of the past mass extinctions to search both for cause of the sudden drops in biodiversity associate with the mass extinctions, as well as examining how the various Earth systems recover.

Here I will discuss new findings about the rate of extinction and recovery at two intervals in time: at the 250 million year old Permian/Triassic mass extinction, and at the 200 million year old Triassic/Jurassic mass extinction. In both cases the biosphere apparently experienced a series of shocks that left earth systems re-equilibrating for millions of years after the extinction event itself. These two events can be compared to the a third mass extinction, that at the end of the Cretaceous.

This type of study is ideal for the integrations of biology and geology within NAI.

Abstract: Virtual Planetary Laboratory

Proposal for a Planetary Atmospheres Database

NASA's Terrestrial Planet Finder (TPF) mission will aim to directly detect and characterize Earth-sized planets around nearby stars. Planets will be evaluated as to whether they are habitable and/or inhabited by life. This evaluation will be limited to an assessment of the photometric and spectral characteristics of the planet, characteristics generally dominated by the atmosphere. Those who plan and run TPF and its projected successor Life Finder, missions that will further the Astrobiology Road Map goals 1, 3 and 7, will need to know how life affects a planet's atmospheric composition. At present, relevant data to address this question are scattered in numerous publications in disparate fields of study. Often these data are in forms that are not readily accessible or even usable by astrobiologists. We propose the development of a database that will collate information relevant to atmospheric planetary science in a manner specifically accessible to astrobiologists. Three types of data would be gathered: spectral, kinetic and historical.

The VPL has started to assemble a spectral database of both stellar and atmospheric phenomena. Stellar observers generally observe in one part of the spectrum at a time. Work involving the whole stellar spectrum must include a search for the various parts. Spectral line-list data for atmospheric components were similarly scattered and will be made available to the scientific community in a coherent fashion as a result of VPL efforts. This database will allow for more complete and accurate models of early Earth's early atmosphere and climate and those of extrasolar terrestrial planets.

Life's impact on a planetary atmosphere is a kinetic phenomenon. Metabolic processes catalyze reactions that otherwise proceed very slowly or not at all at a planet's surface. The rates at which gases are produced and destroyed under conditions found on planetary surfaces must be known to effectively model biogeochemical cycles on Earth and elsewhere. Weathering reactions between microbes, minerals and solutions, including dissolved gases, are of particular interest. Kinetic data are increasingly abundant but there is no central repository and no consensus on how to present these data. It often requires considerable effort to determine if rates of interest are already known or if laboratory work is needed prior to any modeling effort. A systematic assessment of biogeochemical reaction rate data, its availability and the sort of lab work needed to fill in the gaps would be a natural and useful consequence of the proposed database.

Modern-day Earth is the product of over 4 Gyrs of planetary evolution. Knowledge of Earth's previous states, and their global-scale characteristics will directly impact the design of and interpretation of data from TPF, etc. A planetary history database, like the one proposed by Penn State, would provide a coherent set of empirical constraints for theoretical models driven by the spectral and kinetic database components above that were aimed at creating families of probable solutions for the state and appearance of Earth's early environments. To date such models have tended to take a piecemeal approach. A central database would allow for more systematic modeling, by anyone interested in planetary atmospheres and biospheres.

We envision that the NAI EC would recommend the creation of a team that would assemble, validate and distribute the database. The assembly of such a database would help elucidate precisely what laboratory and field measurements are needed in order to test our notions of how planetary atmospheres co-evolve with a planetary biosphere. Database validation and distribution will allow for a common standard of validation and inter-comparison of models and perhaps lower the 'activation energy' for new models.